Lundquist - Exhibit C

on a lamppost, and (b) make an effort to "calibrate" the electrosensitive individual it is utilizing in this manner, and (c) make measurements to locate the boundary of the near field of the transmitter. (It is desireable to employ more than one electrosensitive individual, if the opportunity presents itself to do so, since there will be some variability of response.)

It is also possible to do just (a) and (b), if the City does not want to incur the expense of bringing me in to do (c). Or, if the Health Department has no appropriate instrument to do (b), it could just do (a) alone. But the greatest amount of information would be gained by making a variety of measurements simultaneously.

Such data will provide information as to the size of the *forbidden zone* needed to provide different degrees of protection to electrosensitives. In conjunction with the theoretically calculated size of this zone (which should protect against the elevated cancer risk to which people would otherwise be subjected), this will provide the basic information needed to make a preliminary health hazard evaluation of the plan developed by Ralph Balzano and the cellular telephone companies. On the basis of these data, it should be possible to determine whether this plan requires modification in order to protect the health of the public; and if modification *is* required, there will be quantitative data enabling specific recommendations to be made for the needed modifications.

Obviously, it is in the best interests of all parties—the residents of New York City, the government of New York City, and the cellular telephone companies—to have quantitative information regarding potential health hazards available *before* system details are finalized, site leasing contracts are drawn up and signed, and equipment is installed.

I must tell you that after Arthur Firstenberg told me he was willing to participate in this experiment I had conceived, he had second thoughts about the wisdom of doing so. His reservations are political in nature: he fears that the data so generated may be misinterpreted.

Of course, this is always a possibility. I am trying to persuade him that, rather than cancel his participation in the experiment, we should try to provide adequate safeguards. One such is that all those participating in the experiment receive a *complete* set of all measurements made.

I believe that Arthur Firstenberg can trust the New York City Department of Health to deal with him honestly and fairly; and I believe that the New York City Department of Health can trust Arthur Firstenberg to provide an accurate, honest report of his response to the cellular telephone transmitters during a field test of the type that I believe the City needs to conduct —despite the fact that he is Chairman of the Cellular Phone Taskforce, which is striving to prevent the siting of cellular telephone transmitters on City lampposts. (He can be reached at (718) 434-4499.)

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Arthur Firstenberg's motivation is to protect the health of New York City residents, which is also the mission of the New York City Department of Health. If some solid data can be obtained by co-operation, then it seems likely to be mutually beneficial to work together.

The experiment itself is simple; I outline it on the enclosed sheet. It will be critical to obtain as full a set of information as possible about each transmitter used in the test. For this, the co-operation of Ralph Balzano and the cellular telephone companies will be essential.

I should also emphasize that as various parties review this, they may have ideas as to further information that should be collected in the course of the experiment. I fully expect that some revisions in terms of the information to be acquired may be appropriate; that is why I characterize it as "provisional" on the enclosure.

Based on my conversation with Arthur Firstenberg, I have decided it might be wise to send a copy of my Petition for Reconsideration (submitted to the Federal Communications Commission) to Nancy Jeffrey of your Office of Environmental Epidemiology. Indeed, I think she might be a good choice as the individual within the Department of Health to take charge of this project, if it goes forward.

I hope the New York City Department of Health will be sufficiently interested in protecting the public health to be willing to give the experimental undertaking I propose serious consideration.

Yours for a more healthful environment,

Marjorie Lundquist

Marjorie Lundquist, Ph.D., C.I.H.
Bioelectromagnetic Hygienist

Enc.: Protocol for Field ES Evaluation of Cellular Telephone Transmitters (Provisional)

P.S. If the New York City Health Department would like to educate itself with respect to electrosensitivity, one way would be to subscribe to ***Electrical Sensitivity News***, an international newsletter which I think is quite comprehensive. It is written for electrosensitives, and its editor is an electrosensitive. It began publishing in January, 1996, so back issues are probably available. Write to:

Electrical Sensitivity News
Weldon Publishing
P. O. Box 4146
Prescott, AZ 86302

xc: Enid L. Carruth, Deputy Commissioner
Nancy Jeffrey, Office of Environmental Epidemiology
Arthur Firstenberg

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PROTOCOL FOR FIELD ES EVALUATION OF CELLULAR TELEPHONE TRANSMITTERS
(PROVISIONAL)

1. Complete information must be obtained about each cellular telephone transmitter to be field-tested. This should include (but need not be limited to) the following information:
   Type of transmitter
   Frequency of operation
   Signal: analog/digital
   Spatial orientation of transmitter
   Total radiated power (at time of test)

   Physical location of transmitter (street-corner; lamppost)
   Height of transmitter above ground (sidewalk)

2. A Health Department employee, carrying a convenient distance-measuring device (such as a folding 6-foot carpenter's rule) and either chalk, or tape or sticky labels which can carry a mark, will measure the height of the transmitter above ground, making a mark immediately beneath it on the sidewalk: a "zero" mark. Horizontal distances will be marked off from this "zero" mark.

   If the transmitter radiates a cylindrically symmetrical field and is oriented so that the axis of this cylinder is vertical, the HD employee assumes that the radiated field is the same in all horizontal directions, and chooses a convenient, nearly level direction in which to lay out horizontal distance marks on the sidewalk. These marks indicate where data will be recorded during the experiment.

   If the transmitter does *not* radiate symmetrically in all horizontal directions, then data are to be taken in two perpendicular directions, which are to be selected according to the terrain so that both directions are nearly level.

   If the transmitter is sited on a hillside, and a choice needs to be made as to whether the direction runs up or down hill, an uphill direction should be chosen.

   The space between the distance marks will be arbitrary at the outset; later, experience will dictate an appropriate distance between data collection points. Since the bio-effects may vary with distance in an exponential manner, they need not be equally spaced. But to start out, moving outward from the transmitter, marks may be placed at intervals of 3 feet (or 1 meter) for a distance of about 15 feet, then at intervals of 6 feet (or 2 meters) for another 30 feet.

   The horizontal distance from the transmitter should be indicated by/on the distance mark.

3. The following information shall be recorded about each electrosensitive person employed in this field test: Name (and address and telephone number)
   Height of eyes above ground
   The biological sensor is assumed to be located in the middle of the head, at eye level. Therefore the eye-to-ground distance will be measured.

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4. The test itself will be conducted by having the electrosensitive person walk along the distance-marked path toward the transmitter, beginning at a convenient distance (45 feet, for exmple). The ES person should stop at each marker, standing squarely on it, and should report sensations to the HD employee, who will record it on a data sheet (see below).

   If the HD employee has an instrument with which to make measurements of the electromagnetic field, the measurement should be made at the location of the ES person's head, after this individual has stepped aside. (Any other measurements should be made in the same manner.) Then everyone walks to the next marker and repeats the exercise.

   The transmitter is approached in a step-wise manner, with measurements being made at each marker. The ES person may halt the test at whatever point seems appropriate, as there is no purpose to be gained by this individual becoming ill.

   If there are two or more ES individuals involved in the test, each in turn stands on the marker and gives a report of his/her sensations, taking whatever time the ES individual deems appropriate to do so.

   An acceptable modification of the test procedure is for the HD employee to take an instrumental reading before the ES individual(s) step on the marker, and to repeat this after the ES report(s) are recorded. This will provide a check on the constancy of the instrument and/or field.

5. Desk Calculations. The distance from the ES person's head to the transmitter must be calculated. Where the ground is level, this is a simple exercise in trigonometry or plane geometry. Where the ground is *not* level, U. S. Geological Survey topographical maps of the area will probably need to be consulted, in order to establish the slope of the ground surface.

   Less important, but still of interest, is the angle made by the line connecting the ES person's head and the transmitter, with the plane of symmetry of the transmitter's electromagnetic field (assuming that such a plane exists). The information about the orientation of the transmitter will provide information about the field plane of symmetry, while the line giving the distance from the ES person's head to the transmitter is the one whose angle to the plane needs to be established. This angle will be different for each mark.

   Desk calculations must be done after the marks are established, but may be done either before or after the field test is carried out. Afterward is probably more economical at the outset, because data may not be taken at all marks.

6. The final data set consists of the each ES person's record of sensations, as a function of (a) the distance of his/her head from the transmitter, and (b) the angle of this line with the plane of symmetry of the electromagnetic field of the transmitter. This provides a record of the ES person's response to the transmitter as a function of distance within a cone whose half-angle is the largest angular distance recorded (which will probably be when the ES person was closest to the transmitter).

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7. The date and time of the field test shall be recorded.

8. This is an exploratory evaluation; it is *not* a blind or double-blind test of the ES person's ability to detect electromagnetic radiation. Nevertheless, the HD may wish to extend this test by recording the ES person's responses both when the transmitter is and is **not** operating. This is permissible, within the following restrictions and guidelines.

   (a) The ES person shall **not** be near the transmitter when it undergoes the change from "inoperative" to "operating" status (from **POWER OFF** to **POWER ON**).

   (b) The ES person **may** be near the transmitter when it undergoes the reverse change: from "operating" to "inoperative" status (from **POWER ON** to **POWER OFF**).

   (c) The safety of the ES person is paramount throughout the test. For this reason, it is wisest to take initial measurements with the transmitter operating; then everyone will know how closely the ES person can safely approach the transmitter. The ES person is probably best tested by letting him/her indicate when the transmitter power is turned off. If the HD employee is in telephone contact with a person in charge of the power to the transmitter, and asks the ES person (who may be some distance away) to give a visual signal when the sensation ceases, such as raising his arm, then the HD employee can report on the promptness with which the ES person detected the loss of power reported to the HD employee in the telephone conversation.

FIELD DATA COLLECTION SHEET

Name of Health Department employee in charge of test: _____

Name of electrosensitive individual participating in test: _____

Date of Field Test: _____ Time of Field Test: (from to)

Location of Field Test Transmitter: _____

Transmitter Data

Frequency: _____ Type of Transmitter: _____

Analog/Digital Signal: _____ Total Radiated Power (during test): _____

Spatial Orientation of Transmitter: Height above ground:

Direction of Test Area (relative to transmitter; north, east, south, west): _____

Horizontal Distance	Electrosensitive Person's Reported Sensations	Instrument Reading

[as many blocks as needed for the number of distance marks made]

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EXHIBIT D:

SUPPLEMENT

to

Cellular Telephones and Cellular Towers: Guidelines for Cancer Prevention

by Marjorie Lundquist, Ph.D., C.I.H.

Bioelectromagnetic Hygienist

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SUPPLEMENT

- March 21, 1997 -

to the first (1996) edition of

Cellular Telephones and Cellular Towers: Guidelines for Cancer Prevention

by Marjorie Lundquist, Ph.D., C.I.H.

The first edition was written in mid-1996; this supplement is being attached to bring it up to date. Two new developments necessitate this update. One is an advance in the author's understanding of the cancer risk; the other arises from a new development in the wireless telecommunications industry: the advent of PCS systems.

In the first edition I identified the *near field* of the transmitter as a region of increased cancer risk. It is now clear that the region of greatest cancer risk lies at the *outermost border* of the *near field;* that is, where the *near field* meets the *far field.* This region is also known as the *intermediate field.*

If one were to rank these three kinds of field according to their cancer risk, all other factors being equal, the ranking would look like this:

Greatest cancer risk: *intermediate field*
Moderate cancer risk: *near field*
Lowest cancer risk: *far field*

The cellular telephone system refers to a system of base transmitters and portable hand-held radiotelephones that was first authorized in the USA by the FCC more than a decade ago and which uses frequencies of 800-900 MHz and employs analog (not digital) signals. The risk of cancer discussed in the first edition refers specifically to this system. (The portable hand-held radiotelephones for this system are properly called "cellular telephones".)

Recently, a newer service has become available in the USA, called Personal Communication Service: PCS. This operates at a higher frequency: ~1.9 GHz (~1900 MHz). It employs digital signals, instead of analog. In a PCS system messages are encoded; this ensures that the conversation cannot be overheard, which adds an element of security not present in cellular systems.

Strictly speaking, the portable hand-held radiotelephones used with PCS systems are not cellular telephones, although the public does not distinguish between them and true cellular telephones, calling both kinds "cellular telephones" or sometimes "celphones". There are good reasons for the public to make a distinction, though. One of them is the fact that, due to the different frequencies of the PCS and cellular systems, there is a difference in the cancer hazard to the user of the portable hand-held radiotelephone!

The region of greatest cancer hazard lies at the center of the *intermediate field* that surrounds the transmitter. The center of the *intermediate field* is here designated r_i. This distance in air is calculated according to the formula

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$$r_i = \lambda/2\pi$$

where $\lambda = c/f$. The table below gives values of r_i, the distance from the transmitter at which the center of the *intermediate field* is located, for several different frequencies used with the hand-held radiotelephones of today. The risk of cancer declines slowly as one moves away from the center of the *intermediate field*, in either direction; but the drop in risk is steeper as one moves away from the transmitter toward the *far field*.

f	λ	r_i
800 MHz	37.5 cm	5.63 cm
900 MHz	33.33 cm	5.00 cm
1,900 MHz	15.8 cm	2.37 cm

As the frequency increases, the center of the *intermediate field* moves closer to the transmitter. This means that the region of greatest hazard moves *away* from the user's head as the frequency climbs above 800 MHz! This shift in location of the center of the *intermediate field* causes the risk of brain cancer to *decrease* as the frequency increases above 800 MHz, because the brain increasingly lies in the *far field*, where the cancer risk is comparatively low.

This is why the risk of brain cancer to users of hand-held radiotelephones is considerably greater for the cellular telephones that use frequencies of 800-900 MHz, compared to those of PCS systems that use a frequency of ~1.9 GHz.

Does this mean that the PCS systems that use the 1.9 GHz frequency are safer to use? *No!*

The general rule of thumb is: *the higher the frequency, the greater the hazard to health!* At 1.9 GHz, the most probable long-term health effect that a heavy user of a hand-held radiotelephone will experience is *premature aging of the brain!*

Indeed, this same effect would take place at the lower frequencies, too, though more slowly, if not for the fact that brain cancer will probably *kill* the cellular telephone user long before premature aging of his or her brain has an opportunity to manifest itself!

What does *premature aging of the brain* mean? Think of the elderly: people in their 80s. They are often the target of scams to trick them out of their money, because they cannot use good judgment, as they could when they were younger. That part of the brain that enables people to use restraint—that inhibits the impulse to action—has gone. As a result, the elderly exhibit greater emotional lability than they used to, and they can be persuaded to do foolish things by someone who plays on their emotions. (Also, their memories are poorer.)

People who "get high" by drinking alcohol exhibit the same emotional extremes and lack of restraint that induces foolish behavior, but in their case it is temporary. They can go home and sleep off the effects of the alcohol. When old age does this to the brain, the effect is permanent.

Of course, other health effects, such as early onset of Alzheimer's disease, may also manifest themselves in heavy users of PCS hand-held radiotelephones. There is no way to be certain exactly what will happen, without trying the experiment—which we have now begun to do!

Lundquist

EXHIBIT E:

Paper by Marjorie Lundquist titled:

Evidence of Bias in the Evaluation of Microwave Cancer Data

Lundquist - Exhibit E

EVIDENCE OF BIAS IN THE EVALUATION OF MICROWAVE CANCER DATA

by Marjorie Lundquist, Ph.D., C.I.H.

One of the best laboratory studies ever conducted on the health effects of chronic irradiation of mammals by microwaves was one carried out in the early 1980s on rats; it was sponsored by the U.S. Air Force, and the primary investigator was Arthur W. Guy, of the University of Washington—Seattle. There are several reasons why this is one of the best studies of microwave health effects ever conducted: it was a controlled study; it was a lifetime exposure study; there was an unusually large number of rats in the study (a total of 200); and a large number of variables (155) was measured in the course of the study. Note that the frequency of the microwave radiation was 2.45 GHz—the frequency reserved for microwave oven use in the USA—and the specific energy absorption was just under the maximum permitted by the ANSI standard: 0.4 watts/kilogram.

The official report of this study comprises nine volumes and is many inches thick! Much more widely available is an article[1] by Foster and Guy in **Scientific American**, available in virtually every library, that discusses this issue and this experiment. Both authors are electrical engineers by training. Dr. Guy was the lead investigator for the Air Force study, while Dr. Foster is currently Associate Professor of Bioengineering at the University of Pennsylvania. This paper addresses the content of the **Scientific American** article.

Referring to Dr. Guy's rat study, these authors say [at the bottom of page 37]: "One difference was striking: primary malignant tumors developed in 18 of the exposed animals but in only five of the controls. The probability of such a difference occurring in two samples from an identical population of only 100 animals each is roughly .005, and so the difference is statistically highly significant."

The authors go on to say, "At face value this last finding suggested that low levels of microwave radiation can cause cancer in mice (and by inference in humans). The finding was widely reported by the lay media in 1984 and has been frequently cited in public disputes over proposed microwave facilities. Nevertheless, various considerations militate against drawing a hasty conclusion."

These are discussed in the next few paragraphs, and then the following statement is made: "Our conclusion from these examples and from the large literature on microwaves is that although some hazard from weak microwave fields might be proved in the future, there is currently little evidence for the presence of such a hazard."

Let's take a closer look at how the evidence of a cancer hazard associated with chronic exposure to microwave radiation found in this experiment to be *highly statistically significant* somehow got transmuted into the authors' conclusion that "the finding of excess cancer is provocative, but whether it reflects a biological activity of microwave radiation is not certain."

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EVIDENCE OF BIAS IN THE EVALUATION OF MICROWAVE CANCER DATA

It should be remembered that in *scientific* evaluation of data, the default assumption is that a statistically significant finding at the significance level (*p*-value) chosen at the outset for the statistical evaluation of the experimental data is *always* regarded as evidence of a real effect! An investigator is free to argue against accepting the statistical evaluation at face value, but the burden of proof is upon him to show that there is a good reason to depart from the default assumption.

Consider first the fact that the observed primary malignancies occurred throughout the bodies of the rats, and not in any one particular type of tissue. The authors state, "If some specific type of tumor had predominated, that finding would have made a much stronger case for a carcinogenic effect from low levels of microwave radiation." This statement would certainly be true if the radiation had been aimed at one particular part of the body—for example, the brain. In that case, an excess of primary malignant brain tumors, but not of other types of tumors occurring elsewhere in the body, would have been expected, and finding this expected excess of brain tumors—but not of other types of tumors—would indeed make a strong case for a carcinogenic effect from the radiation.

However, in *this* experiment the irradiated rats were subjected to *whole-body* irradiation: each irradiated rat was placed inside a waveguide through which circularly polarized microwave radiation was passed.[2] Thus no one part of the body was irradiated, to the exclusion of other parts. (The exact circumstances of exposure are not mentioned in the **Scientific American** article. Control rats were similarly placed in sham waveguides.)

Because each irradiated rat experienced whole-body irradiation, there is no *a priori* reason to expect that a carcinogenic effect of this microwave irradiation would manifest itself in an excess of any *one* type of primary malignant tumor. On the contrary, it would be expected that there would be an increase in tumor incidence of like magnitude in *all* the tissues of the body —because all of them had been irradiated in the same manner and to the same degree! And this is *exactly* what was observed!

Why, then, were the authors' expectations so inappropriate to the conditions of this experiment? It seems likely that they were drawing a false parallel with experiments that study the carcinogenic effects of *chemicals*. Chemicals that are taken into the body do tend to be concentrated in specific tissues by the physiological processes that naturally occur in the body. For example, iodine is concentrated in the thyroid gland. Therefore, if radioactive iodine is administered to an experimental animal, the investigator will look at the thyroid for an effect of this radioactivity, simply because the iodine will naturally be concentrated there.

Electromagnetic radiation is a physical agent. It does not consist of matter, and therefore it is not directed to specific body tissues by physiological processes in the same way that chemical agents are. It may be concentrated in certain locations within the body, either deliberately or by reflections from tissues of differing electrical properties, but any such concentration is not going to be specific to a particular *type* of tissue unless the conditions of irradiation were deliberately designed to accomplish this. As has already been pointed out, this was *not* the case in this experiment involving microwave-irradiated rats.

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EVIDENCE OF BIAS IN THE EVALUATION OF MICROWAVE CANCER DATA

A very seductive argument is the one involving the 155 different variables studied in this experiment. "Given such a large number of comparisons, some striking differences would be likely to be found that are in fact merely chance occurrences. The cancer finding may be such a statistical anomaly." Yes, the cancer finding *could* be such an anomaly—but the odds are strongly *against* this possibility! The authors have already said that the whole-body cancer comparison was statistically significant at $p \simeq 0.005$, which means that the probability that this finding does *not* indicate a real difference is about 1 in 200.

What this argument attempts to do is shift the *p*-value selected at the outset as the significance level for evaluation of *each individual variable* studied, and apply it instead to the *collective probability* of a *single* error in the *entire* study! Not only does this force one to employ a much smaller *p*-value when evaluating each individual variable, but it also creates a counter-intuitive situation: the *more* data one gathers and evaluates, the *less* information one ends up with!

Here is another argument for those not yet convinced. Suppose no other effect except the incidence of primary malignant tumors had been studied in this experiment, and the same numbers had been obtained as were reported in this experiment. How would the outcome of the experiment been interpreted? A comparison would have been made and the statistical significance assessed. The outcome would have been statistically significant at a *p*-value of roughly .005 and the difference in cancer incidence between the control and microwave-irradiated rats would have been declared real.

Now suppose the experiment had been done with *one* other variable measured; for example, suppose the animals had also been weighed. How would the cancer data be analyzed now? Because weighing the rats would not be expected to have any effect on the incidence of cancer they exhibit, there would be no change in the way the cancer data were evaluated.

No matter how many variables are added to the study, there is no change in how evaluation of the cancer data is carried out. Each variable is properly evaluated *independently* of all the others; this is the proper *scientific* way to do it.

So the "155 variables" argument is simply a well-disguised argument *against* a scientific procedure for evaluation of the cancer data, and *in favor* of a flawed, unscientific evaluation of the cancer data based on the idea that the more data one gathers, the less one knows!

Finally, let's consider the argument that is based on the number of primary malignancies expected for the particular strain of rat. What this phrase refers to is the accumulated experience with the type of rat used in this experiment, when it is allowed to live a normal life—meaning that it is caged in a laboratory, but is fed a normal diet adequate nutritionally in all known respects, and is not deliberately subjected to any abnormal environmental stressor during its lifetime. The authors argue that the number of primary malignancies occurring in the microwave-irradiated rats in the study sponsored by the U.S. Air Force hardly differs from what would have been expected for the particular strain of rat, implying that there really was *no* difference in cancer incidence attributable to the microwave radiation, after all!

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But there *was* a difference when the irradiated rats were compared with the *control group* in this experiment! This comparison showed that the relative risk attributable to the microwave irradiation was 18/5 = 3.6 =360%, meaning that the cancer incidence in the microwave-irradiated rats was about three and a half times higher than in the control animals!

To do as the authors urge, and make the comparison with the data "expected for the particular strain of rat" instead of with the control group, is to treat this as an *uncontrolled* experiment! But the comparison from an *uncontrolled* experiment is much *less* reliable than the comparison from a *controlled* experiment! Why do the authors prefer the scientifically *less valid* comparison, in preference to the comparison with *greater* scientific validity?

It is reasonable to note the difference between the control group in this experiment and the equivalent data for this particular strain of rat from other sources, and to discuss the reasons for this difference—but the authors don't do this! And here an element of outright deceit enters the picture: the authors knew perfectly well why the control group in this experiment differed from the norm for this particular strain of rat, but they concealed this information from the readers of their **Scientific American** article by making no mention of it!

The fact that they avoided mentioning is this: all the rats in the U.S. Air Force study were *specific-pathogen-free* rats![2,3] These rats were selected *in utero* and delivered by Cesarean into a sterile environment, in which they lived out their lives. Unlike the rats in most such studies, the rats in *this* experiment were *never* exposed to the pathogens—viruses and bacteria—that are normally present in the natural environment! And *this* is why the control group rats in this experiment exhibited a *lower* cancer incidence than was typical for this particular strain of rat! These pathogens play a role in the development of cancer. When they were eliminated from the environment of the rats in this experiment, the cancer incidence of these rats dropped, as indicated by the data on the control group. Exposure to microwave radiation then raised the cancer incidence, and by chance the cancer incidence of these rats was then very close to that typical of unirradiated rats exposed to the pathogens of an ordinary lab-oratory environment.

Drs. Foster and Guy knew this, but failed to share this critical information with the readers of **Scientific American** because they wanted to convince the general public that the microwave-irradiated rats had not *really* experienced an elevated incidence of cancer attributable to microwave irradiation!

Knowing this fact, it immediately becomes evident why the comparison urged by the authors is inappropriate. The only way to avoid falling into the trap set by these authors is to adhere rigidly to standard scientific procedure: compare the irradiated rats *only* with the control group! Then, whether or not critical information is being concealed by a biased investigator, an unwarranted conclusion can be avoided.

Use of specific-pathogen-free rats in this experiment was controversial,[2] primarily because it raised the question of whether the results from this study could be validly extrapolated to creatures, such as man, who do *not* live in a pathogen-free environment. However, use of

specific-pathogen-free rats did remove one potential source of confounding, making the observed evidence of an association of cancer with exposure to microwave radiation that much *stronger!*

Another point is worth mentioning. When a study of this type is planned, a decision is made at the outset not only what data will be collected, but how these data will be evaluated. When statistical evaluation of data is required, as was the case here, the procedure for the statistical evaluation is agreed upon in advance. In particular, the significance level for the statistical tests is decided at the outset. This is done in order to ensure that investigator bias does not enter into the selection of the significance level used for the test.

This study was no exception. The decision made at the outset was to conduct statistical tests at $p = 0.05$—that is, at a significance level of 5%. What this means is that the investigator, Dr. Guy, agreed at the outset that any comparison found to be statistically significant when tested at a 5% significance level would be accepted as evidence of a *real* difference. What actually happened, though, is that after performing the statistical test on the cancer data, Dr. Guy *refused* to accept the results of this test as evidence of a *real* difference in cancer incidence between the two groups of rats!

Had there been some valid basis for this refusal, Dr. Guy could have justified it. But the reasons given were *not* reasons that possessed any validity! Clearly, this was a case of investigator bias!

The funding agency for any scientific study has the responsibility of making sure that the investigators it funds carry out their study in the agreed-upon manner, justifying any departure from this procedure. These psuedoscientific arguments appear in the original technical report as well as in the article in **Scientific American**. Clearly, the U.S. Air Force—the funding agency for this study—had no objection to their inclusion in the original report, because it would have insisted on their removal, if it *had* objected. *What this means is that the U.S. Air Force deliberately permitted a flawed, unscientific evaluation of the experimental data on microwave carcinogenicity to be published under its auspices!*

It has long been noticed that there is a strong bias *against* finding adverse health effects from exposure to microwave radiation in certain quarters. The electrical engineering profession, with the exception of a few specific individuals, is so biased; so are the U.S. Navy and Air Force, both of which rely heavily on radar, which is pulsed microwave radiation.

How very convenient it would be for these branches of the U.S. military if there were *no* evidence of any health hazard from exposure to low-intensity microwaves in the scientific literature! Then residents who object to the erection of radar transmitters near their homes, where residents would be exposed to the beam, because of possible adverse effects upon their health would have no grounds for objection!

As for the electrical engineers, think of the consumer market for microwave ovens following World War II, and for wireless telecommunications now! Think of all the new jobs for electrical engineers created by the new firms (and new consumer products) in these industries!

The experiment discussed here at such length showed a more-than-three-fold rise in the cancer incidence associated with exposure to microwave radiation. Indeed, it indicated the existence of a cancer hazard under energy absorption conditions *permitted by the ANSI standard!* This certainly raises questions about the safety conferred by compliance with this standard, which has long formed the basis for regulation of radio transmitter emissions by the Federal Communications Commission.

The scientific literature is badly "polluted" with this kind of unscientific "whitewashing" of the evidence of health hazardS. So long as the scientific literature remains polluted in this fashion, those who do not want to believe in the existence of a hazard to health from microwave radiation will be able to maintain their position.

What is needed is a careful, *critical* review of the scientific literature, to weed out the bias that is present in the literature, and that obscures the evidence of a health hazard. At present there does not seem to be any funding source willing to sponsor an objective, scientific review of the evidence. There are too many parties who simply *do not want to know* about any health hazards that may be associated with microwave radiation! They prefer to pretend that there are *no* nonthermal hazards to health!

References

[1] Foster, Kenneth W., and A. W. Guy **Scientific American** (Sept. 1986) 32-39. *The Microwave Problem*

[2] Guy, Arthur W., Chung-Kwang Chou, Robert B. Johnson and Lawrence L. Kunz **Proceedings of the IEEE 68** (1980) 92-97.
Study of Effects of Long-term Low-level RF Exposure on Rats: A Plan

[3] Nicholas H. Steneck, **The Microwave Debate**, Cambridge, MA: MIT Press, 1984. Pages 166-169.